The Behavior Analyst 1991, 14, 37–41 No. 1 (Spring)

Humility, Science, and Ethological Behaviorism

William Timberlake and Andrew R. Delamater
Indiana University

We have no argument with the many potential advantages of humility so ably outlined by Neuringer. We also have neither the desire nor the ability to "outhumble" the author who appears to "practice what he preaches" to a rare degree. However, we feel it is important to note some drawbacks to humility as well as to amplify some of the ways in which even a greater humility might improve the status of behaviorism as an account of behavior.

Without doubt a humble behaviorism would raise fewer hackles by appearing less territorial and arrogant. Even a little bit of humility should meliorate inter- and intradisciplinary battles by encouraging an openness to criticism and diversity of approach. Most importantly, as noted by Neuringer, humility should increase the effectiveness of behaviorists as scientists by emphasizing that all knowledge is provisional, and our fundamental assumptions should at least occasionally be reconsidered.

It may be important to add that the encouragement of humility could pay dividends for science in general. In recent history, both Congress and special interest groups have reacted strongly to what they perceive as the arrogance of science. Greater appreciation of their concerns might go a long way toward improving some of the negative attitudes toward science.

Where Less Humility Might Help— Some Advantages to Arrogance

There are, though, obvious drawbacks to humility and advantages to arrogance (in the sense of confidence, assertiveness, and determination). First, to the extent that Neuringer's encouragement of humility occasionally shades into an ethical absolute as opposed to an effective social strategy for advancing scientific knowledge, we have some difficulty endorsing it. An absolute rule requires belief that no matter what the apparent outcome, humility is the correct stance. The history of ethical discourse reveals considerable ambivalence about absolute rules of humility, such as "never criticize" or "always turn the other cheek."

As Neuringer would doubtless agree, there are both analytic and commonsense reasons that an absolute rule of humility might not be the best course of action. In a game theory analysis of conflict situations, the efficacy of a humble approach depends on the relative frequencies of the alternative approaches present in the population. Thus, in the well-known hawk and dove example, a population comprised only of doves can be invaded successfully by hawks, but at the same time an all hawk population can be invaded successfully by doves. The relative effectiveness of the hawk and dove strategies in terms of survival should vary with their relative frequency in the population. The relative effectiveness of arrogance versus humility should vary in similar fashion. The commonsense reasons against absolute humility were summarized in Hoosier humorist Abe Martin's observation, "If the meek inherit the earth, they won't keep it long."

We believe (as probably does Neuringer) that a certain lack of humility is frequently critical in the development of a new point of view. Thus, the experimental analysis of behavior prospered in part because of the gentle but insistent arrogance of Skinner that human behavior could be analyzed most effectively as a product of contingent control by reinforcement. The very success of Skinner's view provided the possibility for Neuringer to call for humility in its practice. In the absence of this success, we would be

Preparation of this manuscript was supported in part by NIMH grant 37892.

tempted to invoke Neuringer's delightful quote from Golda Meir, "Don't be so humble. You're not that great."

In the current state of the world, there are contexts in which we do ourselves no favors by being humble about our science. It has become increasingly popular to declare that science is an enterprise like any other, one determined entirely by the politics of power and privilege. This view has allowed even some scientists to use, in apparent good conscience, any means at their disposal to suppress a particular approach they disagree with. In a similar vein it has become popular in some quarters to view animal learning as having accomplished nothing over the course of its history except the aggrandizement of its followers and the presumably painful deaths of many pigeons and rats through repetitious and largely unnecessary experiments. We don't believe that humility is the answer to such arrogant ignorance.

Most of us discovered as graduate students that politics and power have important roles in science. There are unquestionable fads and fallacies, personal foibles and vendettas, and instances of grave unfairness. But we believe the core of science remains unique, the expectation that the challenge of analytic scrutiny and empirical test will be applied to all current knowledge. It is not sufficient in the long run to reject something out of hand or even simply to act the part of obstructionist on the grounds of religious, ethical, personal, or even methodological distaste. It remains critical to pit one theory, finding, or approach against another which accounts for the same or similar data.

Perhaps most importantly, researchers must have the assurance to value science sufficiently to continue to engage in the process of testing and analyzing data and theories, as well as the humility to recognize when they are wrong. Replication or at least recapitulation remains a critical teaching and learning tool in the process of science. Unfortunately, introductory laboratories less and less often give students exposure to the contingency based control of behavior. Personal humility cannot be permitted to reduce our field to known principles established by famous people. Hands on experience for ourselves and our students combined with a little arrogant questioning is necessary to fulfill the requirements of a vital science.

Where More Humility Might Help— Toward An Ethological Behaviorism

Having claimed a proper place for arrogance and conviction, we'd like to amplify several of Neuringer's examples where we feel that greater humility could be an advantage for a behaviorist approach. In particular we would like to question several prevalent assumptions in behaviorism: (1) that operational definitions of concepts will suffice for a science of behavior, (2) that the application of reinforcement principles can define and account for all forms of purposive behavior, (3) that information from other levels of analysis is of little or no relevance to the experimental analysis of behavior.

Behaviorism arose, in part, as a rejection of introspection and the anthropomorphic interpretation of animal behavior. In Skinner's hands it developed largely as an experimental analysis of functional relations between responses and consequences. Behavior was described and interpreted in the simplest and most overt possible concepts supported by these functional relations. In fact, one of Skinner's conceptual contributions was to see that reinforcement contingencies could be used to define responses and stimuli to avoid the necessity of referring to other levels of analysis or to one's intuition (Timberlake, 1988).

Neuringer notes the relation of this *simple* (operant) approach to Occam's razor and Lloyd-Morgan's canon—basically, make the minimum assumptions necessary to account for the data. This caution was applied particularly to the presumed level of mental functioning underlying behavior. This *simple* approach has been used to argue against internal events as causes, including representations of events and complex mental processes.

Neuringer appears to contend that be-

haviorism's rejection of anthropomorphic approaches was too complete. He suggests that a *similarities* approach may also be useful—the idea that other organisms basically work like humans. He argues that one can combine these approaches to develop a better explanation by working up from the simple approach and down from the similarities approach.

A basic problem with Neuringer's suggestion is that it appears to assume the existence of a single dimension or scale along which we can place animals or their behavior. This scale is anchored at the top by self-observed human behavior and at the bottom by externally defined mechanisms. Objections to the idea of such a scale have been well summarized by Hodos and Campbell (1969).

Further, we see difficulties with both strategies even when applied singly. The *simple* approach assumes that simple mechanisms can be experimentally examined in isolation from the action of more complex processes. The *similarities* approach assumes that animals are basically humans, in particular humans like the experimenter. As an account of an organism's behavior, the *simple* approach is limited by the types of manipulations used by the experimenter and the particular concepts that have been applied to behavior. The *similarities* approach is limited by the self-knowledge of humans and their tendency to project their own feelings and beliefs on other organisms.

A humble behaviorism should make clearer contact with the nature of the subject than does either of these views. To achieve this end we support development of an ethological behaviorism in which the behavior of an animal is viewed and interpreted in terms of the structures and functions evolved to fit the animal to its niche (e.g., von Uexkull, 1934/1957). Instead of projecting oneself as a particular type of human into the circumstances of the organism, one attempts to assume both the circumstances *and the characteristics* of the organism.

Information gained from observation, ecology, physiology, the analysis of sensory, perceptual, memory, and motor systems, and the operating characteristics of motivational states is relevant and necessary for an ethological behaviorism. Experimenters not only need to put themselves in the subject's shoes, they need to wear them—walk, watch, hear, touch, and act like the subject. The humility required to assume this role coupled with the power of the experimental approach should increase the efficiency with which the understanding, prediction, and control of behavior can advance.

In keeping with an ethological behaviorism, we would like to see more humility concerning the presumed role of reinforcement in accounting for purposive behavior. The obvious power and the apparent operational clarity of reinforcement techniques has obscured an equally important determinant of responding, namely the organization of behavior prior to the imposition of reinforcement contingencies. The potential power of reinforcement technology has led to the assumption that reinforcement not only entirely causes the effects we see in the laboratory, but similar operations have shaped the organism from its birth to the present. In the extreme version of this view, similar reinforcement operations have shaped the evolution of the animal (e.g., Skinner, 1966).

We feel this view goes too far. As pointed out elsewhere (Timberlake & Lucas, 1989), both in evolutionary time and in development, organisms regulate ingestion, metabolism, elimination, and aspects of reproduction before operant learning appears to enter the picture. In evolutionary history, learning most likely arose as an addition to an already functioning organism, an addition that favored differential survival through learned adaptation to the environment.

This view suggests that a fundamental contribution of evolution to current learning is an organization that promotes learned (purposive) behavior appropriate to the organisms's evolutionary niche and survival. For example, attempts to reinforce a wide variety of responses with the same reinforcer have met with varying success (e.g., Shettleworth, 1975; Timberlake, 1990). A most important factor determining ease of success in op-

erant conditioning appears to be the functional relevance of the instrumental response to the motivational state related to the reinforcer.

However we choose to conceptualize learning, our view will be informed by an understanding of the relation of learning to the characteristics and ecology of the organism. Without recognition of preorganized response components, stimulus processing, and interacting motivational states, we cannot hope to account fully for variations in the sensitivity of responses to reinforcement and for the form which the response takes. Establishing the nature of such response, stimulus processing, and motivational organization can also help clarify the role of learning in regulatory and timing relations (e.g., Lucas, Timberlake, & Gawley, 1989), including learning related to circadian periodicities. An understanding of the regulatory systems of the organism is apparently required even in the simplest task of identifying ahead of time a reinforcer that will work reliably in a new situation (Timberlake & Farmer-Dougan, in press).

Finally, an ethological behaviorism is compatible with allowing information and results from different levels of analysis to contribute to our analysis. One of us (WT) remembers, in a graduate seminar, giving a report on the fascinating experiment in which Don Blough produced pigeons that distributed their inter-peck intervals exponentially. At the end of the report I asked what seemed to be an obvious question—how did this happen? By what mechanisms could reinforcement produce an exponential distribution of pecks? Was an exponential distribution a basic characteristic of the mechanisms of peck emission? Was it the same set of mechanisms that produced the distribution of pecking under variable-interval and fixed-ratio responding? What were the limits on producing these different distributions of inter-peck intervals?

The answer from the seminar was that reinforcers had produced this distribution by means of differential reinforcement of the inter-response times, and it was best not to look beyond the reinforcement procedures and the data. To me this answer meant giving up the possibility of greater understanding, prediction, and control of the phenomenon in the mistaken belief that knowledge of data from other levels would contaminate rather than improve a behavioral analysis.

A similar limitation appears to be occurring with respect to the potential contributions of event representations and complex associative structure in determining behavior. The notion that stimuli are encoded in a form that can be modified through further experience and can affect subsequent responding has been heuristic and important in accounting for results in both Pavlovian and operant procedures (Colwill & Rescorla, 1986; Dickinson, 1989; Rescorla, 1990). Discomfort with words such as representation and association is not a sufficient reason to ignore the increasingly interesting and complex results obtained by experimenters working in associative and information processing traditions.

To be sure, it is provocative and fun to show by clever demonstrations that common tendencies to attribute mental functioning to animals can be unnecessary (e.g., Epstein, Lanza, & Skinner, 1980). However, demonstrations are not the touchstone of science; experimental analysis is. It is one thing to argue that science can be conducted solely at the level of operations and overt behavior, but it is another to ignore the possibility of improving this analysis by incorporating information and perhaps concepts from other levels.

From an ethological view, organisms should differ in the complexity and form of their covert mental life. It seems likely that species that must determine well ahead how to distribute behavior over variable alternative rewards may be able to create relatively long-term plans based on long-lasting memory. Visually guided social creatures capable of complex social relations may well have the ability to imagine the consequences of their social behavior and physical appearance. Species that have few predators and diverse

food stuffs may react with curiosity in many circumstances (Glickman & Sroges, 1966). The primary danger in invoking mental life in other animals is the tendency to over-interpret based on perceived similarity to our own mental life and our assumptions about the connection of our experiences to our overt behavior. It is here that a humble ethological behaviorism coupled with the unique power of the experimental approach may well help rescue us, even from ourselves.

Summary

Arrogance and combativeness is a sure path to resistance and has doubtless been responsible for more than a few of the difficulties the field of behavior analysis and science in general have encountered. On the other hand an inappropriate humility is a sure course toward being ignored and our science disregarded. More humility of the sort espoused above and in Neuringer's article coupled with a less humble commitment to the scientific process could go a long way toward advancing the analysis of behavior beyond its promising beginning.

REFERENCES

Colwill, R. M., & Rescorla, R. A. (1986). Associative structures in instrumental learning. In G. H. Bower (Ed.), *The psychology learning and motivation* (Vol. 20, pp. 55–104). San Diego, CA: Academic Press.

Dickinson, A. (1989). Expectancy theory in animal conditioning. In S. B. Klein & R. R. Mowrer (Eds.), *Contemporary learning theories: Pavlovian conditioning and the status of traditional learning theory* (pp. 279–308). Hillsdale, NJ: Erlbaum.

Epstein, R., Lanza, R. P., & Skinner, B. F. (1980). Symbolic communication between two pigeons (*Columba livia domestica*). *Science, 207,* 543–545.

Hodos, W., & Campbell, C. B. G. (1969). *Scala Naturae*: Why there is no theory in comparative psychology. *Psychological Review, 76,* 337–350.

Glickman, S. E., & Sroges, R. W. (1966). Curiosity in zoo animals. *Behaviour, 26,* 151–188.

Lucas, G. A., Timberlake, W., & Gawley, D. J. (1989). Learning and meal-associated drinking: Adjustments in post-prandial drinking following meal-related deficits. *Physiology & Behavior, 46,* 361–367.

Rescorla, R. A. (1990). Evidence for an association between the discriminative stimulus and the response-outcome association in instrumental learning. *Journal of Experimental Psychology: Animal Behavior Processes, 16,* 326–334.

Shettleworth, S. (1975). Reinforcement and the organization of behavior in golden hamsters: Hunger, environment, and food reinforcement. *Journal of Experimental Behavior: Animal Behavior Processes, 1,* 56–87.

Skinner, B. F. (1966). The phylogeny and ontogeny of behavior. *Science, 153,* 1205–1213.

Timberlake, W. (1988). The behavior of organisms: Purposive behavior as a type of reflex. *Journal of the Experimental Analysis of Behavior, 50,* 305–318.

Timberlake, W. (1990). Natural learning in laboratory paradigms. In D. A. Dewsbury (Ed.), *Contemporary issues in comparative psychology* (pp. 31–54). Sunderland, MA: Sinauer Associates.

Timberlake, W., & Lucas, G. A. (1989). Behavior systems and learning: From misbehavior to general principles. In S. B. Klein & R. R. Mowrer (Eds.), *Contemporary learning theories: Instrumental conditioning theory and the impact of biological constraints on learning* (pp. 237–275). Hillsdale, NJ: Erlbaum.

Timberlake, W., & Farmer-Dougan, V. (in press). Reinforcement in applied settings: Figuring out ahead of time what will work. *Psychological Bulletin.*

Uexkull, J. von. (1957). The worlds of animals and men. In C. H. Schiller (Ed. and Trans.), *Instinctive behavior: The development of a modern concept* (pp. 5–81). New York: International Universities Press. (Translated from Uexkull, J. von. (1934). *Streifzuge durch die Umwelten von Tieren und Menschen.* Berlin: Springer).